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OBSERVATIONS

ON THE

DOCTRINE

OF

PHLOGISTON,

AND

The Décomposition of Water.

Part II.

By JOSEPH PRIESTLEY, LL. D. F. R. S. &c. &c.

Sed revocare gradum ———

Hic labor hoc opus est.

VIRGIL.

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OBSERVATIONS

ON THE

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PART II.

THE INTRODUCTION.

I THINK myself happy in having already drawn a considerable degree of attention to the two opposite theories of chemistry by my late publication on the subject, and I am therefore encouraged to endeavour to keep up this attention a little longer, and, if possible, till the question now depending be decided to general satisfaction. At present I am sensible that I shall be considered as very obstinate, in not admitting the new theory, when the old one is almost universally abandoned ; though it is not true, that I am the

only person who adheres to it. Mr. Kirwan informs me, that Messrs. Crell Werntrumb, Gmelin, and Mayer, men of considerable reputation in Germany, still maintain the doctrine of phlogiston. So, I also hear, do my friends of the Lunar Society of Birmingham, among whom Mr. Keir has given as much evidence of his judgment in these subjects as any other person whatever. And I see by the advertisements of books, that there is in France itself a recent publication against the new theory.

As truth can never suffer, but must always gain, by investigation, I shall not offend any rational advocate for the antiphlogistic theory, if I endeavour to point out in what respects the replies that I have already heard of to my late publication appear to me to be unsatisfactory; and though I have given as much attention to them as I can, they appear to me far from unexceptionable. But my distance from the centre of philosophical information lays me under great disadvantages in this respect, as well as many others.

All

All the answers to my book that I have yet heard of are that of Mr. Adet in French, the Monthly and Analytical Reviews of it in England, and that of Dr. Maclean, Professor of Mathematics and Natural Philosophy in the College of New Jersey. But as all these writers agree, as far as they go, together, I may presume that other answers will go on the same general principles; so that in replying to them I may be replying to others also. I shall not, however, think the controversy closed, till I hear from Mr. Berthollet and the other French chemists, to whom my Treatise was addressed.

In matters of much nicety, as the subjects of many of my numerous experiments are, I do not always expect to escape the charge of inaccuracy, and perhaps of inconsistency. Persons who, from a want of experience, are not sufficiently aware of the difficulties, will not have the candour that the circumstances call for. From such I must appeal to the judgment of those who have the requisite experience and qualifications. I will,
however,

however, venture to say, that no person who has made near so many experiments as I have, has made so few mistakes. I do not mean with respect to *opinions*, but in my reports of *facts*. But after all our care, errors will sometimes arise from a want of attention to small differences of circumstances; and no person can keep his eyes open to every thing that is before him at the same time.

SECTION I.

Of the Solution of Iron in the Vitriolic and Marine Acids.

THE most simple of the experiments that I have proposed for discussion, with a view to decide concerning the merits of the two theories in question, is that of the solution of iron in the vitriolic and marine acids. Here the question to be solved is, from which of the substances present comes the *inflammable air* that is procured in the process.

cess. The phlogistians say it comes from the iron, and the antiphlogistians from the water. But to this I have objected that, since, according to their own hypothesis, water consists of about six times as much oxygen as it does of hydrogen, there must be a large deposit of oxygen in the vessel, and that I cannot find it there. That it is not in the acid appears, as the antiphlogistians themselves say, by its saturating no more alkali after the process than before. They, therefore, say, and there is no other alternative, that this addition of oxygen is in the iron.

But I now ask, How does this appear? If there be any addition of oxygen in this case, it must shew itself either by an addition to the acid, or by its being exhibited in the form of dephlogistified air, called by them *oxygenous gas*. The former is not pretended; and so far is the latter from being true, that if the precipitate be exposed to a red heat, it yields much less pure air than the same quantity of the acid without the iron would have done.

For

For this purpose I took as much vitriolic acid as I had found in the experiment recited in Vol. III. p. 197. of my *Observations on Air*, (in three vols.) to have yielded 130 ounce measures of dephlogisticated air, of the standard of .15, which is extremely pure, and saturated it with iron. But after this it yielded only 52 ounce measures of air, of the standard of .55, which is much less pure. This shews that this precipitate is so far from containing more oxygen, that it contains less than the acid. It is in reality possessed of the opposite principle, which is agreeable to the phlogistic theory. For since much more inflammable air is procured from iron by means of steam only, than by its solution in any acid, more of the principle of which inflammable air consists, viz. phlogiston, must adhere to this calx of iron than to the other.

Dr. Maclean says, p. 19, “ There is the
 “ most satisfactory evidence that iron, after
 “ its solution in sulphuric acid is in a state
 “ like that of the black oxyd, or finery cin-
 “ der.” But the dephlogisticated air which

is yielded by this precipitate is all procured before it comes to this form of a calx. After it becomes black, in which state it ought to contain more oxygen in proportion to its bulk than before, it yields no oxygenous gas at all. Also, neither in this, nor in any other state, will it oxygenate muriatic acid, which however easily dissolves it. It therefore shews no sign of its containing any oxygen at all. The new theory, however, requires that it be dignified with the appellation of the *black oxyd of iron*. The black oxyd of manganese gives more evidence of its right to the name they have given to it.

I have no great objection to admitting that this precipitate from the solution of iron in the vitriolic acid, when it is burned black, is the same substance with finery cinder. Both in this form, and in that of a brown powder, this precipitate has several of the same properties with those of finery cinder. They neither of them either gain or lose any weight by exposure to the greatest heat. When heated in atmospheric air, they both

diminish and, as I usually say, phlogisticate it, though very slowly. They also equally imbibe inflammable air when heated in it, but with this difference, that the production of water seemed to be greater in the reduction of finery cinder than in that of this precipitate. But the experiment being of no great consequence, I did not give much attention to this circumstance.

There is something very extraordinary in the theory of this oxygen attaching itself to the iron on its solution in an acid. Mr. Adet says, p. 60, “ Experiments prove that
“ metals, in order to be combined with an
“ acid, require to be united with oxygen ;” and explaining himself farther, he says, “ In
“ reality, a metal not combining with acids
“ but when it is in a state of oxide, and not
“ passing into this state but by its union
“ with oxygen, must necessarily absorb oxygen in order to unite with the acid. But
“ this oxygen can only be supplied by one
“ of these two substances, the acid itself, or
“ the water which it contains. If the oxygen
“ had

“ had been given by the acid, it would have
 “ been in part decomposed, and would in
 “ consequence have saturated less alkali. But
 “ since it saturates the same quantity of al-
 “ kali, it has not been decomposed.”

On this I would observe, that if the separation of the oxygen from the water, in order to its attaching itself to the iron, take place prior to its solution in the acid, that solution is not necessary to its producing inflammable air ; and this effect would in all cases be produced by some affinity between the iron and the oxygen in the water only. If the affinity be between the iron and the oxygen universally, what could prevent the iron from saturating itself in the first instance with that which belongs to the acid, as well as with that which was a constituent part of the water, in which it is at least much less evident. I would also ask, if an acid will not dissolve iron till it be oxydated, but will do when it is, why will not the acid of vitriol dissolve the black oxyd of iron, or finery cinder, more readily than it

does iron ; since in this substance it finds the iron already abundantly oxydated ; and yet the reverse of this is the case.



SECTION II.

Of Finery Cinder.

THE great question between the advocates for phlogiston and their opponents is, whether the substance that has usually been called *finery cinder*, which is formed by the contact of steam with iron when it is red hot, be a proper *oxide of iron*, that is, whether it contain any principle which can be exhibited either in the form of an acid, or of dephlogisticated air ; and yet this, which is the only proper evidence in the case, has not been given. To say that it forms *water* when heated in inflammable air, and that water cannot be formed without oxygen, is taking for granted the very thing to be proved ; since the water so procured, I say, is that which was imbibed by the iron, and
is

is now expelled on the introduction of the phlogiston with which it had parted.

One of my arguments to prove that finery cinder contains no oxygen is, that when it is dissolved in marine acid, it does oxygenate it. Let us, however, hear the account that my opponents give of this circumstance. Mr. Adet says, p. 55. "The nonoxygenation of the muriatic acid by the solution of finery cinder is owing to the latter retaining the oxygen so strongly, as not to be disengaged by the action of heat, aided by the attraction of the muriatic acid." To this I answer, that if the acid had not been able to dissolve this substance, this might have been said with some degree of plausibility; but since it does dissolve it completely, so volatile a thing as oxygenous gas, of which it is supposed to contain so large a quantity, and with which this acid has so strong an affinity, could hardly escape being evolved.

Dr. Maclean makes very light of this, as indeed he does of every other difficulty.

"It

“ It certainly” he says, p. 10, “ does not
 “ follow that because muriatic acid can se-
 “ parate a certain quantity of oxygen from
 “ lead, when this is combined with a great
 “ quantity of that substance, that it should
 “ likewise separate oxygen from iron, when
 “ this is united to a comparatively small
 “ quantity.” But finery cinder, if, as all
 antiphlogistians say, it owes all its additional
 weight to the pure oxygen, which it gained
 from the water which it had decomposed,
 must contain much more of it than lead in
 any state, or indeed than any known sub-
 stance in nature. For the addition to its
 weight is nearly one third ; whereas the ad-
 dition to the weight of lead by making it
 into minium, is only about one tenth of its
 weight. Can this be all pure oxygen, that
 the iron acquires, and yet not oxygenate
 muriatic acid ?

He farther says, p. 24. “ The antiphlo-
 “ gistians suppose the addition made to
 “ iron to be oxygen, because the compound
 “ resembles in every respect, as Dr. Priestley,
 “ himself

“ himself allows, that substance which is
“ formed by burning iron in oxygenous gas,
“ or in atmospheric air. And this they con-
“ sider as an oxyd, because while it is form-
“ ing the oxygenous gas disappears, and its
“ weight is exactly equal to that of the iron
“ and oxygen consumed.”

But it is evident to me, that though the pure air, or oxygen, disappears in this process, it is not imbibed by the *iron*, but only the *water* which was its base, and which formed at least the principal part of its weight; the pure air, or oxygen, serving to form the *fixed air* which is always found in this process, and which cannot have any other origin. Consequently, the calx of iron so formed when heated in inflammable air gives out nothing but water. The quantity of fixed air produced in this process appears to me to be quite sufficient to take all the pure air that disappears in it. It is possible, however, that a small quantity of oxygen may enter the iron along with the water to which it was united; as few substances are perfectly

perfectly separated from each other by any chemical affinity.

When spirit of salt is distilled over a quantity of scales of iron, which, being made in the open air, are most likely to have some of this principle attached to them, it has something of that faint smell which a very small quantity of dephlogisticated air will give it. But it is the more evident from this, that if this species of finery cinder had contained any considerable quantity of oxygen, it would have been extricated in this process. That a little, and not more, appeared, I consider as a proof that it contained no more; whereas, according to the new theory, it must contain more than any other substance.

A comparison of the effects of the application of spirit of salt to finery cinder, and to red precipitate, is much in favour of the former containing no sensible quantity of oxygen. This acid presently deprives the precipitate of its colour; during which a

great degree of heat is produced, and the smell of the dephlogisticated acid is pretty pungent, though it soon becomes faint. When, after this, it is exposed to the heat of a burning lens in confined air, the vessel is filled with dense white fumes; but when the substance becomes dry, it recovers its red colour, and the air is increased. But when the acid is applied to finery cinder, there is no heat, and little or no smell; and when it is heated in confined air, the air is diminished. Can both these substances, which when treated in the same manner exhibit such different phenomena, be equally oxyds?

That a very small quantity of oxygen is attached to the scales of iron, I have thought probable from a barely perceivable quantity of fixed air which I have found when they are revived in inflammable air. But so small a quantity as this makes nothing for the new theory.

Dr. Maclean farther says, p. 28, “The
“quantity of carbonic acid formed by the
“combustion of iron in oxygenous gas is
c “very

“ very trifling, and this is owing partly to
“ the gas containing some before the opera-
“ tion, and partly to the plumbago contain-
“ ed in the iron.” Now this, I will venture
to say, cannot possibly be the source of the
fixed air which appears in this process. If
the air before the process contained any sen-
sible quantity of fixed air, it could not fail
to appear on its transmission through lime-
water. I appeal to the experience of any
unbiaſſed experimenter in this case againſt
the declaration of Mr. Berthollet, or any of
the defenders of the antiphlogiſtic ſyſtem
whatever ; and Dr. Maclean, I preſume, only
writes after them ; for he never once refers
to any experiments of his own.

The quantity of *plumbago* in the iron that
is uſed in this experiment, and which this
process could not diſengage from it, could
not, if it was wholly fixed air, yield a hun-
dredth part of that which is produced. There
is nothing whatever, concerning which, I
am, from much experience, better ſatisfied
than I am of the truth of theſe obſervations.

What

What makes it almost a certainty that the water which is found on the revival of finery cinder in inflammable air has not the source that the antiphlogistians suppose, is the great difference in the quantity which is found in this case, and that of the revival of other calces in it. Dr. Maclean says, p. 11.

‘ When oxyd of mercury is reduced in hydrogen gas, that disappears, no oxygen gas is obtained, but a quantity of water may be collected.’ Now I am confident that no person who had ever seen the experiment could have written this. The quantity of water that appears in this case is barely perceivable, being no more than sufficient to constitute the base of the inflammable air imbibed by the calx, or that might have been concealed in the substance operated upon; whereas when finery cinder is revived in the same circumstances, the water forms itself into hundreds of small drops, which unite, and run down the inside of the vessel in all directions.

Now if this water was really formed by the union of the inflammable air in the vessel

fel with the oxygen expelled from the calx, they ought surely to unite in the same proportions, to form the same thing. The antiphlogistians themselves always say, that the proportion of hydrogen and oxygen in water is universally 15 parts of the former to 85 of the latter. Here, therefore, is much more water produced than their principles can account for. The same quantity of inflammable air disappears, but the same quantity of water is by no means formed. The obvious conclusion therefore is, that in the case of the calx of iron, the great quantity of water produced was simply expelled from the calx when the inflammable air was imbibed; whereas the calx of mercury contains little or no water to be expelled, and only unites with the phlogiston in the inflammable air that disappears.

Before I conclude this section concerning finery cinder, I must take notice of what Dr. Maclean too confidently advances about it. "The Doctor," he says, p. 26, "is certainly mistaken in supposing that finery cinder cannot rust. Mr. Fourcroy says it
"rusts

“ rusts sooner than common iron, and every
“ apothecary knows it does so. If the rust
“ of iron be made red hot in a retort, a
“ quantity of carbonic acid is disengaged
“ from it, and the iron remains in a state
“ of black oxyd. The rust, therefore, is a
“ *carbonate of iron*, and must contain all the
“ principles which compose the black oxyd,
“ and therefore can contain nothing capable
“ of excluding that which would convert it
“ into rust.” This very confident assertion
would astonish me if it were not too much
of a piece with the rest of the Doctor’s per-
formance. In direct contradiction to what
he asserts, I still say that finery cinder is not
subject to rust. In England no use having
been made of it before it was attended to by
my brother-in-law, Mr. John Wilkinson,
(one of the most intelligent and successful of
all the iron-masters in that or any country),
but to mend the roads, it has lain in heaps
for years, I may even say ages, without ac-
quiring the least tinge of brown. All my
specimens have ever remained free from rust,
and the physicians, who are also apotheca-
ries,

ries, in this place, assure me they never saw or heard of any such thing. They get it from the blacksmiths in the form of *scales of iron*, and the blacksmiths say the same. It must, therefore, as I have observed, be saturated with some principle very different from that of the common rust of iron, and is by no means the same thing, notwithstanding what Dr. Maclean says to prove the contrary.

He also considers the rust of iron as containing more oxygen than finery cinder. But, though I do not know exactly what addition of weight iron acquires by being converted into rust, it cannot, I am confident, be near so much as it acquires by passing into the state of finery cinder. If, therefore, as the antiphlogistians assert, all the additional weight be oxygen, finery cinder must contain more of it than the rust. But neither of these substances, whether they contain more or less of oxygen, will oxygenate muriatic acid. Nor what I think of no less consequence, will finery cinder (which,
if

if it contain any oxygen, contains the most of it) when revived in inflammable air, produce any *fixed air*, as the revival of minium, which contains much less oxygen, in the same circumstances does.

SECTION III.

Of the Calces of Mercury.

THE phlogistic theory, I readily acknowledge, is most pressed by the phenomena of the calces of mercury. But in forming any general theory we must content ourselves with the fewest difficulties. It will hardly be pretended by the greatest admirers of the antiphlogistic theory, that it is attended with none. Those which attend the phlogistic with respect to these calces I do not think to be insuperable, and farther experiments may throw more light upon them.

It is always asserted by the antiphlogistians that the calces of mercury are revived not only

only without addition, but without loss. This, however, I have never found to be the case, and after many trials, often assisted by other persons, I have concluded that, after the solution of mercury in the nitrous acid, there is a loss of one twentieth of the whole. And I must still say that there are calces of mercury which certainly imbibe inflammable air, and therefore that this substance, or the base of it, phlogiston, exists in that metal as an element. This is true both with respect to red precipitate, and turbith mineral.

In reviving red precipitate in inflammable air, I find no sensible quantity of water, of which there appears abundance during the revival of finery cinder in the same circumstances, but I sometimes get fixed air. Mr. Adet says, p. 64, "The fixed air which is generally obtained by the revival of red precipitate in inflammable air, comes from the carbone held in solution in that air." But it cannot be proved that this kind of air ever holds any carbone, or any element of fixed air, in solution. That which some-

times appears on the decomposition of it, when it is fired with dephlogisticated air, is in some cases certainly, and therefore in all the others probably, formed by their union in the explosion. For in some cases, I have shewn, that the quantity produced is so great, as to exceed the weight of all the inflammable air employed; so that its being supposed to consist wholly of fixed air will not solve the difficulty.

As to the calx of mercury from the acid of vitriol, Mr. Beaumé *, I find, agrees with me in the observation, though I did not know it at the time, that it is not completely reducible by mere heat. But “later observations,” Dr. Maclean says, p. 11, “shew that the turbith mineral, or any other substance into which it may be converted

* With Mr. Beaumé I was a little acquainted. Mr. Macquer introduced me to him in his laboratory in Paris, and though he was an avowed opponent of the whole of the pneumatic chemistry, he was a good operator in the old way, and his fires, I am persuaded, were as hot as any raised by the persons mentioned by Mr. Adet, or those by Dr. Hope.

“ by a red heat, does not require any addition to constitute it a metal.” And Mr. Adet says, p. 43, “ that the yellow oxide of mercury has been revived without addition by Messrs. Monnet, Bouquet, Lavoisier, and Fourcroy.”

To this I can only say, that I have never been able to reduce the whole of this calx by any heat that I could apply, not even that of a burning lens of sixteen inches diameter; and this, I am confident, is a greater heat than can be raised by any furnace whatever. From being a red friable substance, this heat converts it into a yellowish glass, with the loss of about three-tenths of its weight; but after this, no continuance of the same heat makes any farther change in it. Yet after this, when it is heated in inflammable air, the air is imbibed, and it is covered with a black powder, evidently *ethiops mineral*, into which mercury, with all its component parts, whatever they be, is known to enter. This substance also, and not directly running mercury, was frequently
the

the result of my experiments on this precipitate before I left England.

I wish that Dr. Maclean would repeat this experiment himself, as well as others which are differently related by myself and my opponents. Whatever is asserted by any anti-phlogistian he never hesitates to admit; but he makes no difficulty of disregarding any thing that I assert to the contrary. This is certainly an experiment of considerable consequence. For if it be true that inflammable air be really imbibed by any calx of mercury, that it is revived by it, and cannot be revived without it, we are authorized to say universally, that some element of which it consists, and no doubt phlogiston, is a necessary component part of that metal, and therefore of all the other metals also.

In contradiction to what I and Dr. Withering have said of mere heat not being able to separate fixed air from the aerated barytes, Dr. Maclean says, p. 50, “ Dr. Hope has discovered that it can be done

“ by such a temperature as can be raised in
“ a smith’s forge.” This, however, I will
venture to say could not be done in Bir-
mingham, where the forges and furnaces are
as good as those of Edinburgh.

In reply to what I have observed of water
being essential to this kind of air, because
readily procured with it, and not at all with-
out it; he says, p. 50, “ He has entirely
“ overlooked the property which carbonic
“ acid gas has of dissolving water. Every
“ chemist knows it has this property, and
“ in a greater degree at a high than at a low
“ temperature. But water is not necessary
“ to the constitution of this gas, because it
“ exists before the solution of the water,
“ and may be deprived of water by the sul-
“ phuric acid, or any deliquescent substance,
“ and still remain carbonic acid gas.”

Whether Dr. Maclean will allow me to
know what every chemist knows, or not, I
was not ignorant of, nor did I overlook, the
property of fixed air, or of any kind of air,
dissolving

dissolving water. But that vitriolic acid, or any other substance, will deprive that, or any kind of air, of *all* the water which it only holds in solution, is more than any chemist can pretend to know. But this is nothing to the purpose. I find no air at all, nothing in the form of air, without the application of water, a great quantity of which disappears in the process, and can only remain in the air. I therefore conclude that water is essential to this kind of air. I speak from my own observations, and I only wish that Dr. Maclean would speak from his. If he have no aerated barytes, I will supply him with some for the experiment.

SECTION IV.

Of the Composition and Decomposition of Water.

I WISH I could say that I have met with any thing in Dr. Maclean's Observations on my Experiments relating to the Composition and Decomposition of Water, besides general exclamations, some false assertions, and much boasting of the superior accuracy of the French chemists. "In what respects," says he, p. 45, "his experiments were less liable to exception than those of the French chemists, is what I do not comprehend. "Theirs were performed on a very extensive scale, great care was taken to ascertain the degree of purity of the gasses before combustion, and the apparatus was so constructed, that the results could be determined with the greatest nicety. The Doctor's, on the contrary, were made with very trifling quantities of materials, their purity was not tried, and their weight not accurately determined."

Let

Let us now consider what these high sounding words amount to. Experiments made with a great quantity of materials are not, always on that account, the most accurate, especially where, as in this case, the thing to be determined is simply the *quality* of the result. When I can produce but a few drops of a strong *acid*, and as often as I please, from the very same materials from which I am told that I ought to get only *pure water*, what is it to me whether they produce gallons?

Great care, he says, was taken to ascertain the purity of the gasses, wherein with respect to me, he says, the purity was not tried. Now that of mine was not only tried, with as great accuracy as they could try theirs, but the dephlogistified air that I used was purer than any that I believe they ever pretended to have made. For with two equal measures of nitrous air, the residuum was only four hundredth parts of a measure, and this slight impurity was certainly not in the dephlogistified, but in the nitrous air, which
is

is very apt to vary in its quality, and very difficult to obtain pure. And yet with this very pure dephlogisticated air, and a proportion, exactly defined, of the purest possible inflammable air, I got drops of a stronger acid than can be procured by means of air less pure. To this impurity, viz, a mixture of phlogisticated air, the antiphlogistians always ascribe the production of the acid, though if the air be purposely less pure, I never fail to find that impurity, viz. the phlogisticated air, unaffected by the process; so that it could not possibly have contributed to the production of the acid.

With the greatest confidence, however, Dr. Maclean says, p. 53, “the dense acid
“vapour that I produced by the explosion
“of the two kinds of air was occasioned by
“the azote contained in the oxygenous gas
“that I employed.” He might as well have said it was occasioned by that which I did *not* employ. If ten times the quantity of azote in the air I used had been wholly decomposed, it would not have amounted to

the hundredth part of the weight of the acid that I procured.

Their apparatus, he says, was so constructed, that the result could be determined with the greatest nicety. On the contrary, it was extremely complex, as a view of their plates will shew, and mine was perfectly simple, so that nothing can be imagined to be less liable to be a source of error. How, indeed, was this possible? I use only one large vessel, of glass, or copper. I put into it at once a certain proportion of the two kinds of air, the purity of which, when it is necessary, I can ascertain as well as other persons. From the simplicity of the apparatus no other substance can possibly mix with them, and I then explode the whole at once by an electric spark. After this I presently find the result by examining the liquor that is drained from the vessel. Though I have not gallons of this liquor, I have some ounces, which no antiphlogistian would care to drink. Will Dr. Maclean say that my process is less accurate than that of the French,

because it can be finished in less than five minutes, and theirs requires the assiduous attendance of some days.

Using the same most simple apparatus, I can, by only varying the proportions of the two kinds of air, produce the result which the French chemists so much boast of. For I can produce water as free from acid as theirs, and with much greater certainty, as I have no attention to give to a flame, lest it should at any time burn too fiercely. But in this case I always produce a quantity of *phlogisticated air*, in which they acknowledge that the principle of acidity resides. They also do not deny that they had a surplus of the same kind of air; and as to the quantity of it, I cannot help supposing that, interested as they were to make it as little as possible, being men, and of course liable to the biases of other men, they may have represented it, by the allowances they made in their computation, something less than it really was. All the inside of my large vessel being, of course, wet with the liquor produced

duced by the explosion, I could not pretend to *weigh* that which was drained from it with much accuracy. But then very little depended upon the *quantity*, compared to the consideration of the *quality* of the liquor; and this may be as clearly ascertained by drops, as by the largest quantities; and till the French chemists can make their experiments in a manner less operose and expensive, requiring fewer precautions, and less of computation, I shall continue to think my results more to be depended upon than theirs.

That phlogisticated air *can* be produced from the same materials from which I get nitrous acid, viz. dephlogisticated and inflammable air, I have given various and sufficient proof. Dr. Maclean, however, says of them, and of other of my experiments, p. 66, “As the Doctor has not favoured us with a
“detail of his experiments, and as they bear
“the most striking marks of not having been
“performed with accuracy, I will not take
“up your time” (speaking to his pupils)
“with a review of them.”

Though an account of the experiments to which he here refers was not inserted in the pamphlet on phlogiston, it was printed for the Transactions of the Philosophical Society at Philadelphia, which I expected would have been published long ago. It is evident, however, that Dr. Maclean had seen a copy of those articles. How else could he say that they bear such evident marks of not having been performed with accuracy? He ought certainly to have shewn *how* they could have been made with more accuracy, with respect to the proper object of them, and I request that he will do it.

Notwithstanding this authoritative condemnation of those experiments, on which, however, till I hear some good reason to the contrary, I shall continue to lay some stress, I shall here give an account of another experiment, though I do not pretend to say that it is more accurate than the rest. Having made a number of pieces of iron rusty by dipping them in marine acid, I put them into a glass vessel, which I then filled up with mercury, and I displaced this mercury by inflam-

inflammable air. After waiting about eight months, I examined the air, and found it to be very slightly inflammable, the far greater part of it being evidently phlogisticated air. The iron, from being red, which all antiphlogistians will say was owing to its containing oxygen, was become black, being covered with a kind of soot, which was easily wiped off, staining the fingers and paper. Under this coating the iron was of its usual colour.

Whence, now, came this phlogisticated air, if not from the union of dephlogisticated and inflammable air? I have pretty clear proof of the same elements forming in other circumstances fixed air, especially the production of a great quantity of this kind of air from heating a mixture of iron filings and red precipitate; so that, in contradiction to the maxim of Mr. Lavoisier, this *carbonic acid*, as it is called, is formed without *carbone*. This remarkable fact I am told is disputed by the antiphlogistians, but I have lately repeated the experiment with the same result as before.

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This experiment is very little liable to the objection of the Monthly Reviewer, p. 371, as the pieces of iron had not been exposed to the atmosphere any great length of time, and I am confident that by no process whatever could any phlogisticated air have been extracted from them.

If the above-mentioned black substance with which the pieces of iron were coated be *plumbago* (and of this little doubt can be entertained) it will appear to be a calx of iron supersaturated with phlogiston, and that the whole of the iron might have been converted into it, but that *plumbago* cannot be contained in iron, so as to yield, on its solution in an acid, the phlogisticated air of which my opponents have endeavoured to avail themselves.

As to the experiments recited in my third section, I shall not enlarge upon them at this time, but leave my readers to compare them with the remarks that have been, or may be made upon them, and judge for themselves.

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